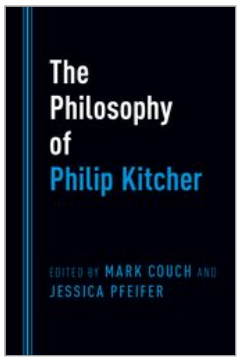
Bringing real realism back home



The Philosophy of Philip Kitcher
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Bringing Real Realism Back Home

A Perspectival Slant

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Abstract and Keywords

Michela Massimi attempts to rescue Kitcher’s real realism from an inadequacy she believes it faces. She argues that his use of the distinction between idle wheels and working posits is inadequate in certain cases as a reply to Laudan’s historical argument against realism. Her perspectivalism, she maintains, provides the real realist with a preferable route to defending realism, since it does not privilege our own perspective. Where Kitcher relies on our own perspective to pick out those parts of theories that are deemed true from our own perspective (the working posits), her perspectival realism identifies claims that we have reason to believe are true since they are justifiably retained in the shift from the original perspective to the perspective(s) from which they are assessed.

Keywords: idle wheels, Laudan, perspectival realism, Philip Kitcher, realism, real realism, working posits

1. Introduction

When it comes to debates on realism in science, Philip Kitcher’s (2001a) “Real Realism: The Galilean Strategy” (henceforth abbreviated *RR*) occupies its well-deserved place among my top five must-read articles

published in the past forty years or so on the topic, alongside Putnam's (1975) "What Is 'Realism'?"; Boyd's (1991) "Realism, Anti-Foundationalism and the Enthusiasm for Natural Kinds"; Laudan's (1981) "A Confutation of Convergent Realism"; and Psillos's (2000) "The Present State of the Scientific Realism Debate." Personal as this top-five list may be, there is no doubt that "Real Realism" has ushered in a silent revolution. Without much fanfare it has shown that realism is hard to resist because it "begins at home" and "it never ventures into the metaphysical never-never-lands to which antirealists are so keen to banish their opponents" (*RR* 191). Kitcher has taught us how realism began with homely considerations such as those used by Galileo to persuade the Venetians about the reliability of his telescope to spot ships approaching the harbor. The following step, from "being a reliable naval instrument" to "being a reliable instrument, in general"—capable of revealing the craters of the Moon, the satellites of Jupiter, and the phases of Venus—was a short one.

(p.99) The Galilean strategy that Kitcher has so admirably defended in "Real Realism" against both empiricism and constructivism (in their respective semantic and epistemic forms) entices us to a "homely line of thought" and warns us against any "Grand Metaphysical Conclusions." Its impact cannot be underestimated. We all stand on Galileo's shoulders with our defiant trust in science and technology to give us access to nature and its innermost secrets (pace empiricists' intimations against 1-kilogram mortar and King Kong's ability to break it). More to the point, we all stand on Kitcher's Galilean grid in thinking of realism as a "homely" enterprise, where a *divide et impera* strategy of "working posits" and "idle wheels" can guarantee to the selective realist a cornucopia of past scientific results. Where to go from here?

Closer "home" is my reply. Whose home? The very home from which Kitcher (1981) began his intellectual journey in the early 1980s, with his reflections on explanatory unification as the battleground of two grand traditions: the Aristotelian tradition, whereby "scientists aim to fathom the order of being, an order that is typically opposed to the order of knowing;" and the Humean tradition (continued by Mach, Duhem, and the logical empiricists), which, on the contrary, argued for "no joints at which nature can be carved, no objective necessities, no mind-independent causal connections" (Kitcher 1986, 202). Against both traditions Kitcher defended a *via media*, leading out from Kant's writings on the methodology of science onto the philosophy of science. Central to the Kantian project envisaged by the early Kitcher was an analysis of scientific knowledge and objective understanding that "does not depend on any mind-independent notions of causation, natural

necessity, or natural kind" (204). Yet fifteen years later, in "Real Realism," Kitcher took a stance against the Kantian tradition—in its epistemological constructivist outfit—holding that "the realists' world is an inaccessible realm of noumena" (*RR* 188).

In this essay I suggest bringing real realism closer home, namely back to its Kantian roots. The very same roots that make real realism a "homely" kind of realism, against any Grand Metaphysical Conclusions about the world, its causal necessities, and natural kinds. In particular I suggest reinterpreting a key aspect of real realism—that is, the notion of success at stake in "working posits"—along more "homely" lines, lines that acknowledge historical continuity, conceptual nuances, and our role as epistemic agents in assessing success and inferring truth. (For some preliminary reflections, see Massimi 2012, 2014.) The result is a form of *perspectival realism*—to adopt Ron Giere's (2006, 2013) terminology—which is, however, already at a distance from what Giere himself intends by this term (p.100) (see Massimi 2015a). Hence my very own (loosely Kantian-inspired) *perspectivalist* slant to real realism.

Key to the Galilean strategy—as I see it through Kantian lenses (see Massimi 2010)—is not just to deploy the telescope to overcome fictitious boundaries (i.e., those between sea and land, Venice and Amsterdam, Heaven and Earth) but also to approach nature through principles of reason in one hand and "experiments thought out in accordance with these principles" in the other hand, "yet in order to be instructed by nature not like a pupil, who has recited to him whatever the teacher wants to say, but like an appointed judge who compels witnesses to answer the questions he puts to them" (Kant [1781–87] 1997, Bxiii–xiv). This is how Kant famously portrayed Galileo's contribution to bringing natural science onto the secure path of knowledge "after groping about for so many centuries." It is this *further* Galilean strategy that I turn my attention to here.

I cannot do justice to the breadth of the philosophical arguments that Kitcher's real realism has put forward. And much as I'd like to discuss Kitcher's articulated response both to the epistemological empiricism of van Fraassenean flavor and to the epistemological constructivism of Kantian descent,[1] I have to leave those for another occasion. For here I concentrate on Kitcher's influential response against the "blockish holism" of epistemological empiricism in its historical form (best expressed by Laudan 1981), which seems to assume that "a theory is false because it is not entirely true" (*RR* 170). In reply real realism insists "that the past successes stem from parts of the theories that are approximately correct," (*RR* 170) namely from those hypotheses that

are genuinely put to work (i.e., that characterize “working posits”), and are as such “approximately true.”

In section 2 I review Kitcher’s famous distinction between working posits and idle wheels in the context of his realist defense against the challenge coming from the history of science. In section 3 I focus on the notion of scientific success and distinguish between two variants: success “from within” and success “from above.” In section 4 I suggest a perspectivalist take on real realism in the form of a notion of success “from within,” able to assess success from a human vantage point and to capture truth across scientific perspectives. I conclude by considering possible objections and replies to the perspectival view canvassed in section 4.

(p.101) 2. Against the “Blockish Holism” of Epistemological Empiricism: Working Posits and Theoretical Excrescences

A powerful line of argument against realism has traditionally rehearsed a seemingly compelling historical point against the “success to truth” inference: that similar inferences “made by our predecessors would have issued in conclusions we now take to be quite wrong” (*RR* 168). Let us leave aside whether the list of past successful yet false theories reflect historical records or abide instead by the antirealist inclination to inflate examples. There are undoubtedly “prominent cases from the history of science in which views we now take to be false were genuinely successful by anyone’s standards” (168). Not surprisingly perhaps, Fresnel’s wave theory of light is one such favorite example. No matter how false the ether theory is, Fresnel’s ability to use his mathematical equations to predict a bright spot in the middle of a dark shade won skeptics like Poisson in the Paris Academy of Sciences and belies epistemological empiricists’ objection to realism. To the eyes of real realists, Fresnel’s wave theory of light is successful not in virtue of a tenuous distinction between structure and substance (pace structural realists). Instead its success was achieved via “approximately true descriptions of some of the features of light waves (the mathematical accounts) while being wrong about others” (*RR* 170) (i.e., how light waves propagate in the ether). The real realist sees Fresnel as employing many tokens of “light wave” to refer to electromagnetic waves and “as saying a large number of approximately true things about the properties of electromagnetic waves of the appropriate type,” despite the false opinion about the propagation of the waves through an elastic ether (*RR* 170).

Against the blockish holism of the antirealist that would invite us to regard as false a past theory that is no longer true by our own standards, the real realist recommends a *divide et impera* approach.

Working posits are approximately true as long as they explain why past theories were successful (to the extent that they were), while *idle wheels* are "theoretical excrescences that are incorrect" (*RR* 170) and often entangled with working posits. Fresnel might not have distinguished between the two, but it is not a foregone conclusion that it would have been impossible for him to do so. Contemporary selective realists have made their own the real realist's distinction between working posits and idle wheels. And the distinction continues to be, in my view, one of the most persuasive replies against antirealist challenges coming from the history of science.

However, a difficulty still awaits. For the objection against the structural realist—that the structure/substance dichotomy cannot easily be (p.102) exported to other examples—can similarly be leveled against the real realist. Consider, for example, Aristotle's theory of free fall as accelerated motion toward a natural place. The theory was undoubtedly successful by its own lights at the time, and it provided a springboard for medieval commentators (from Simplicius to Hipparchus and the Arabic commentators), whose views fed into the impetus theory of Buridan and Oresme, and ultimately into Galileo's early Pisan studies on free fall (see Massimi 2010, 2015b). What are the working posits in Aristotle's theory of free fall? And where do theoretical excrescences begin? Was Aristotle's hypothesis that bodies get heavier nearer the Earth, an "idle wheel"? Well, it provided an explanation—in Aristotle's own scientific perspective—of why free-falling bodies accelerate (as opposed to decelerate or move with constant speed) when moving toward their natural place (where, he assumed, bodies would regain their "form"). Moreover it suggested that there might have been forces acting on the body and pulling it either toward its natural place or in some different direction (what Avicenna and Abū'l-Barakāt called natural and violent *mail* and what Buridan called impetus, as an intrinsic force due to a natural gravity, which was in turn the ancestor of the early Galileo's *gravitas* as a weight-related concept and ultimately of Newton's gravitational mass). Was there *anything* approximately (or even *remotely*) true in Aristotle's theory? Or should we conclude that Aristotle's theory was *quite simply* false? What has gone wrong with this example?

3. Success from Above and Success from Within: A Further Thought on Galileo's Strategy

Here is a possible diagnosis. In replying to Laudan's challenge to convergent realism, the real realist has himself employed too stringent a criterion in the *divide et impera* strategy: a criterion of success "from above" rather than "from within." In Fresnel's case, current electromagnetic theory provides the criterion of success to discern between the working posits of Fresnel's theory (i.e., the equations for polarization by reflection, which still bear his name) and the idle wheels of the ether theory (long gone from contemporary textbooks). In Aristotle's case, the criterion of success "from above" cannot similarly be deployed to distinguish between working posits and idle wheels. Too many centuries separate the Galilean-Newtonian theory of free fall from Aristotle's, and all the conceptual nuances, small theoretical steps, and turning corners that the notion of free fall underwent in that span have long been forgotten (were it not for the assiduous work of dedicated historians of science).

(p.103) It may well be that Aristotle's theory is as promising as Fresnel's when it comes to identifying parts of the theory that are essential to success (working posits), and hence approximately true. Scientists in Hipparchus's time (or even in Buridan's time) might still have been able to identify such parts. But *we* no longer are, because two millennia separate us from Aristotle. Thus if my diagnosis is correct, there is nothing wrong with the real realist's *divide et impera* strategy. What has gone wrong instead in the example of Aristotle's free fall is the real realist's tacit appeal to the scientific/convergent realist's criterion of success "from above," that is, from *our very own current vantage point* as if that vantage point were the best one to assess the past, or the one that provides a royal road to Truth with capital T.

The perspectival realist (of Kantian leaning) enters the scene. For the perspectival realist (of the kind I like) would rejoin that there is no privileged vantage point from which to assess scientific claims of the past. Our current vantage point is not a disguised Nagelian *view from nowhere*, providing special epistemic standards for assessing the past or a privileged access to the ontology of nature. Our current scientific perspective is only one among many others that our ancestors have happened to occupy and from which failure and success can be evaluated. Homely perspectival considerations of this kind invite us to embrace a more modest criterion of success from within when it comes to discerning between working posits and idle wheels.

The perspectival realist may adopt a Galilean strategy, namely the one adopted by Galileo in his early treatment of free fall in the Pisan treatise *De Motu antiquiora* (ca. 1590s), before he discovered the law of free fall ($s: t^2$). Against Aristotle's cause of motion (i.e., motion toward a natural place), Galileo looked for the "true cause" (*vera causa*) of accelerated motion in an Archimedean theory of buoyancy that could explain why bodies move up or down. But the analogy with Archimedean buoyancy could explain only uniform (not accelerated) motion. Hence Galileo had to resort to the medieval impetus theory of Buridan and Oresme in thinking of a weight-related concept of gravity (*gravitas*) as an *internal static force* that would decay during the free fall. Galileo's *momentum gravitatis* (sometimes also referred to as *impeto*) is already at a distance from medieval impetus theory, as it is from Newton's gravity, understood as an external impressed force acting at a distance between two bodies. Galileo's gravity is still a weight-related internal force, compared to Newton's thoroughly dynamical concept of gravity. Yet Galileo had to rethink the medieval concept of an internal force and make it obey "indubitable principles" so as to demonstrate the law of free fall. (For full details of this story, I refer the reader to Massimi 2010.)

(p.104) Galileo's kinematic studies exemplify the perspectivalist strategy of engaging with the past from within (rather than from above). Working with the Aristotelian tradition that goes from Hipparchus to the Arabic commentators and Buridan and Oresme's impetus theory, Galileo could operate within well-trodden paths. He could resort to Archimedes's buoyancy and Hipparchus's theory of free fall and introduce gradual changes to key concepts. For example, the change from impetus as an internal force propelling a body to *momento* (*momentum gravitatis*) as an internal force that, after having propelled the body, would gradually decay, causing the body to acquire degrees of speed (*celeritatis momenta*) in its descent. Galileo's breakthrough about free fall did not happen by debunking the Aristotelian tradition (pace Galileo's own rhetoric against Simplicius in *Two New Sciences*). Nor did it happen by selecting working posits in the Aristotelian tradition, for even Archimedean buoyancy and Hipparchus's theory were inextricably entangled with idle wheels and not amenable to being imported tout court into the Galilean story.

Instead the Galilean kinematic strategy consisted in small theoretical steps and subtle conceptual nuances that ultimately allowed Galileo to turn the corner from the Aristotelian tradition. Galileo's ability to interrogate nature with principles of reason on the one hand (i.e., the indubitable principles from which he demonstrated the law of free fall)

and with experiments thought out in accordance with these principles on the other hand (i.e., both thought experiments with chords and real experiments with inclined planes) made the revolutionary shift possible. The perspectival realist can appeal to *this* Galilean strategy to bring the real realist's notion of success back home: from above to within.

4. Success and Truth across Scientific Perspectives

But how should the perspectivalist notion of success from within be understood? So far I have simply suggested that it should not be understood as the ability of inquirers to identify parts of a theory that are essential to success and hence approximately true. But this can hardly be enough to understand the perspectivalist move I am suggesting for real realism. We need to unpack the slogan.

In what follows I take my cue from broader discussions on perspectivalism in contemporary epistemology to propose that success from within should be understood as success with respect to standards of performance adequacy appropriate to the scientific perspective of the inquirer *when* (p.105) *assessed from the point of view of another (either diachronically subsequent or synchronically rival) scientific perspective*.[2]

Given the Aristotelian-Archimedean perspective and the available evidence for free fall, Galileo could conclude that the Aristotelians failed to satisfy standards of performance adequacy appropriate to *their own* perspective in the explanation of the phenomenon. For example, Aristotle's theory could not explain the precise mechanism through which equal degrees of speed accrued during the descent, and hence why motion toward a natural place was *uniformly* accelerated motion. The Aristotelians could not obviously be blamed for having believed what they believed about free fall (i.e., that it was motion toward a natural place) given their own scientific perspective. And the proposition that free fall was motion toward a natural place cannot be regarded as relatively true (i.e., true for the Aristotelians but false for Galileo), on pain of abandoning realism altogether for alethic relativism.

Scientific perspectives, I suggest, provide *contexts of assessments* for scientific claims. Galileo could assess the Aristotelian claims about free fall and find them lacking in satisfying what, from Galileo's own perspective, were the standards of performance adequacy *appropriate to the Aristotelian epistemic context* (e.g., Why is free fall as motion toward a natural place accelerated motion and not uniform motion, as one should expect from the analogy with Archimedean buoyancy?). In answering these questions Galileo came eventually to establish a new

scientific perspective, from which it became possible to evaluate new claims about free fall. His scientific perspective, in turn, can be found lacking in satisfying what, from our own current perspective, are the standards of (p.106) performance adequacy appropriate to the Galilean-Newtonian epistemic context (e.g., How to think of Galilean free fall when sense impressions about the free mobility of rigid bodies and paths of light rays get called into question, as they were with Helmholtz's mirror sphere thought experiment, for example? What becomes of Galilean-Newtonian gravity in a non-Euclidean space?).

Success from within is then the ability of a theory to *perform adequately* with respect to standards that are appropriate to the theory's wider epistemic context—or scientific perspective, as I prefer to call it—when *assessed from the point of view not just of the scientific perspective at stake but, crucially, from the point of view of other scientific perspectives*. Building on recent important work in epistemology,[3] I suggest the following definition.

A scientific claim (*SC*) meets the criterion of success from within iff:

> (a) *SC* expresses a proposition *p* at scientific perspective SP_1
> (b) *p* is true (i.e., corresponds to states of affairs in nature) *and* meets standards of performance adequacy in SP_1 when assessed from other scientific perspectives SP_2, SP_3, SP_4 …

This definition of success from within vindicates the real realist's expectations for successful posits to track truths in nature (via the first part of premise (b)). Yet it is *perspectival* in giving up on both a Nagelian view *from nowhere* and a convergent realist's/real realist's view *from here now*: it does not take our currently successful scientific claims as the gold standard for assessing past failures and successes.

Success from within is kosher to the Kantian spirit of perspectivalism in giving due consideration to epistemic agents' (or, I should say, scientific communities') commitment to scientific claims (without dismissing them out of hand as sheer errors of the past). Success from within does justice to historians' anti-Whiggish plea for judging past theories in their own terms and *by their own standards* (not by ours) *when assessed from the point of view of other (diachronically subsequent or synchronically rival) scientific perspectives*. At the same time, it avoids the perils of truth relativism by anchoring (p.107) success to the truth of perspective-independent states of affairs. That free fall is accelerated motion is a perspective-independent state of affairs that either holds in nature or does not. But that accelerated motion is, in turn, motion toward a natural place (as opposed to motion due to a force of gravity)

is a scientific claim that can be assessed only within a given epistemic context, with its standards of performance adequacy, and so forth. At the same time, standards of performance adequacy *in and of themselves* cannot be entrusted with the goal of delivering success from within.

For example, it is not enough for Aristotelians to be satisfied with their own theory of free fall (and their associated standards of performance adequacy) for it to count as successful. Scientific communities cannot ratify their own success if their practices are not deemed successful by other communities of inquirers. Yet other communities cannot in turn smuggle in their own standards of performance adequacy when evaluating other (past or rival) theories. A scientific claim proves inadequate (and hence unsuccessful) when the content of the claim is false *and* it fails to meet its own standards of performance adequacy *when assessed from another perspective*.[4]

It was possible for Galileo to assess Aristotle's theory of free fall by Aristotle's own standards (expressed by Simplicius in *Two New Sciences*) and conclude about its inadequacy. As it was possible for William Thomson (later known as Lord Kelvin) in 1847 to assess Carnot's cycle by Carnot's own standards (which included conservation of caloric) and conclude about its inadequacy (when combined with Joule's claim that a quantity of heat proportional to the mechanical work produced must be consumed in a paddle-wheel experiment).[5]

(p.108) These examples show important features about the definition of success from within I just gave:

1. The relevant standards of performance adequacy for scientific claims are settled in the original *context of use*, that is, in the scientific perspective in which the claim is first formulated and advanced.
2. Subsequent perspectives provide *contexts of assessment* from which it is still possible to evaluate past scientific claims *by their own original standards.*
3. Given the richer informational content available to subsequent perspectives, it may be possible for later assessors to regard the performance adequacy of past claims as lacking in some respects; hence it is possible for later assessors to either retain or withdraw (in whole or in part) past scientific claims on the basis of their continuing performance adequacy.

Success from within then becomes a commitment that a community of epistemic agents undertakes to *retain* past scientific claims when their performance adequacy continues to be regarded as satisfactory *from the point of view of later scientific perspectives*. Success from within bears important similarities with the real realist's working posits. Both react against the blockish holism of epistemological empiricism. Against scientific realism they both invite us to a more nuanced reappraisal of past theories. Against structural realism they both refrain from cashing out success in terms of structure versus substance. And both equally stress what might be called the *enactive* nature of scientific success: success is whatever works or continues to perform adequately.

Yet real realists and perspectival realists differ when it comes to the notion of success. For real realists deploy working posits to identify hypotheses that are approximately true by the criterion of success from above. Whereas perspectival realists of the kind I like would urge to deploy success from within to identify scientific claims that—by being *justifiably retained* in the shift from the original perspective/context of use to another perspective/context of assessment—we have reasons for thinking of as true (to the best of our knowledge). The perspectival slant I am offering to the real realist's working posits is then in terms of commitment of a scientific community to assess and justifiably retain past scientific claims whenever their performance adequacy continues to be deemed satisfactory by their own original standards when assessed *from the vantage point of other perspectives*.

On this perspectival reading truth is not an ex post facto explanation of the ongoing success of some scientific claims. Instead truth is built into (p.109) the aforementioned definition from the ground up, with the first conjunct of premise (b): "*p* is true." It is the truth of the propositional content of a scientific claim *together with* the ability of the claim to meet standards of performance adequacy at SP_1 (when assessed from other scientific perspectives) that ultimately ground success from within. And I do not mean "*explanatorily* ground" it. I mean instead that "*ontologically* ground" it. If the propositional content of the claim were false by realist lights (i.e., if there were no such a thing as *p* in nature), even if the claim were hypothetically able to meet standards of performance adequacy when assessed from other perspectives, the claim would not qualify as successful under the criterion of success from within.

Imagine a scientific community in the eighteenth century that could have built a perfectly consistent scientific system around caloric to advance various claims about the production of mechanical work,

thermal expansion, and matter's states of aggregation, among others. Our best eighteenth-century scientists failed to distinguish among states of aggregation as physical in nature (and went on to identify water as a *liquid* chemical substance; see Kuhn 1990). But let us assume that our hypothetical community can do better than our own Lavoisier, Dalton, and Carnot and come up with a perfectly good system of knowledge around caloric that meets their own standards of performance adequacy at the time.

For example, such system proves consistent when offering explanations in terms of caloric for matter's states of aggregation and the production of mechanical work; it gives simple and elegant accounts of how caloric (by being released and absorbed) underlies all these phenomena; it seems accurate with respect to the evidence available to the community at the time; and so on. Should we not judge—from our own current perspective—such a community as having met its own standards of performance adequacy? Should we not assess its scientific claims as being successful, despite their propositional contents (in terms of caloric) being false? More to the point, who are *we* to conclude that their propositional content is indeed *false*? Are not we reintroducing a much-dreaded view from nowhere to reach such a cross-perspectival Grand Metaphysical Conclusion?

5. Objections and Replies

Not so fast. What needs be considered in this imaginary case is whether positing caloric can indeed give rise to such a perfect system of knowledge able to meet all the aforementioned standards by the light of the hypothetical eighteenth-century community. I contend that it cannot. (p.110) *Consistent explanations* first. Assuming caloric is an imponderable fluid—as eighteenth-century scientists did and our hypothetical scientists would presumably also do—would immediately pose severe challenges to any attempt to provide a consistent explanation of mechanical work and states of aggregation. Mechanical work would require caloric to be consumed (pace conservation of caloric), as much as turning water into ice would require removing caloric (qua a shell of imponderable fluid surrounding water's particles) and yet expanding the overall volume. How can water's particles lose part of their volume (by releasing caloric), while also expanding their overall volume? Caloric does not seem to license consistent explanations.

Simplicity next (a notoriously slippery standard, if any). Would caloric provide a simple and elegant account of various phenomena? Caloric could be squeezed out of particles' volumes (assuming a Daltonian model) and get reattached to them at ease. As simple as that? Well, assuming some mechanism was in place to explain what held caloric attached to the particles of matter, what had the power to detach it from matter and reattach it at will, and so forth. Perhaps some attractive and repulsive forces might do the trick. Or perhaps electrical fluids. Or some ethereal substratum (along the lines of Kant's matter of heat). Simplicity is not within easy reach. A complex story would have to be told about the mechanisms underlying caloric's behavior in all these phenomena, mechanisms that can potentially be at odds with each other.

Perhaps *accuracy* with the available evidence fares better than consistency and simplicity when it comes to standards of performance adequacy. Let us assume our hypothetical community has produced a system of scientific claims that are accurate by the experimental standards available to the community at the time. Such claims must surely be regarded as successful (no matter how false caloric is from our current vantage point). An analogy may help here. Suppose I have an accurate story about hedgehogs living in my garden and creeping out at night to collect the mulberries that have fallen on the ground. My story is so accurate that it tells me with precision that hedgehogs come at night, between 1 and 2 a.m., from the far right corner of the garden,

behind the hedge, and collect only the juiciest mulberries they can get their spiky claws on. So my available evidence of red mulberries on the ground seems to support the accuracy of my story.

But is accuracy such a malleable standard? Surely, even my garden hedgehogs would have to respond to some mundane questions: Do they come out every night? From 1 a.m. or from 2 a.m.? What about the purple spots on the ground that look like old juiciest mulberries getting moldy? Accuracy (be it the accuracy of a measurement or the accuracy of a scientific claim) comes always in tandem with other standards, such as consistency, (p.111) fruitfulness, and explanatory power. Our imagined eighteenth-century caloric supporters would have to tell a pretty convincing story about how their scientific claims involving caloric were accurate over and above fitting a sample of observed regularities (especially if such sample proved in conflict with others, and the caloric mechanisms envisaged in each case were in contradiction with each other and hard to pin down).

Whatever the standards of this hypothetical eighteenth-century community could have been (the list above, of Kuhnian flavor, is only illustrative and is not meant to be exhaustive), the examples should make it clear that building a system of scientific claims on an ontologically false ground is not going to go very far. The system of claims would soon fail *by its very own standards of performance adequacy*. And we do not have to resort to hypothetical scenarios. Real historical communities that entertained standards similar to the ones listed above came to realize the inadequacy of the caloric theory in the nineteenth century.

Let us take stock. A critic was envisaged that challenged the criterion of success from within on the ground that as long as a community can justifiably be regarded as meeting its own standards of performance adequacy, the scientific claims advanced on behalf of such standards should count as successful (despite their propositional contents being false). This objection attacks the realist component in my definition of success from within, namely the first conjunct in premise (b): "*p* is true." For it would seem possible for *p* to be false and yet still meet standards of performance adequacy in *a given scientific perspective* so that scientific claims about *p* would count as successful (despite *p* being false). In response I have shown how if *p* were false, it would prove in practice impossible to justifiably meet standards of performance adequacy in a given scientific perspective. And for good reasons too: *ex falso quodlibet*. An ontologically false ground (e.g., caloric) cannot possibly license scientific claims that are arguably consistent, simple, accurate, and so on (unless inconsistency, inaccuracy, and so forth are

themselves acceptable standards within a particular scientific perspective).[6] Thus I conclude that if the propositional content of the scientific claim were false by realist lights (i.e., if there were no such a thing as *p* in nature, and hence the first conjunct of my premise (b) did not hold), it would in practice be impossible for the claim to meet standards of (p.112) performance adequacy in its own scientific perspective (and even more so when assessed from other perspectives). In other words, it is not the case that *p* is false *and* nonetheless meets standards of performance adequacy in a given perspective. A scientific claim of this kind would not satisfy the criterion of success from within.

A different kind of worry may be raised at this point. Isn't the truth of the propositional content *p enough* to secure success from within? Aren't the standards of performance adequacy themselves idle wheels, not necessary to secure success? Here a different critic is envisaged, who may retort that a real realist's working posits ultimately underpin the truth of *p*, and my definition of success from within collapses onto the real realist's success from above at a closer inspection. The critic may insist that Fresnel's theory worked and proved successful not because it met standards of performance adequacy in Fresnel's time (e.g., it was fruitful in predicting novel phenomena; it seemed accurate in explaining polarization by reflection; and so forth) but because Fresnel's "light wave" referred to electromagnetic waves of high frequency. Or better, Fresnel's theory met those standards *because* its working posits (i.e., electromagnetic waves) were true. And to emphasize the idleness of the standards of performance adequacy themselves, one could easily invoke consistency with the ether theory as an example. (Yes, Fresnel's theory was consistent with popular ether theories at the time, yet consistency *in and of itself* does not cut any ice for the success of Fresnel's theory.)

In reply one may consider what would happen to a lone researcher who gets it right without yet meeting the standards of performance adequacy of her community at the time (perhaps because such community has not quite gotten to the stage of precisifying standards able to capture the truth of what the lone researcher has just discovered). Should we conclude that the researcher has been successful? Here I cannot help but share Richard Boyd's negative answer to this question,[7] although I give a perspectival gloss to what he portrays as the social dimension of scientific inquiry. That "*p* is true" is not sufficient by itself to ontologically ground success, *unless p also* meets standards of performance adequacy at SP_1 when assessed from another scientific perspective.

Consider the astronomer V. M. Slipher, who, at the Lowell Observatory in Arizona throughout 1912–17, was able to measure with precision the radial velocity of galaxies and to empirically establish that galaxies were (p.113) expanding a decade before Hubble found the law for this phenomenon, and at a time when Einstein was introducing his cosmological constant in the equations of general relativity to secure a static universe.[8] Slipher was the lone brilliant experimentalist who got it right in a scientific perspective dominated by general relativity with Einstein's and de Sitter's interpretation of the field equations as implying static solutions. It was only in 1924 that Friedmann first, and then Lemaître in 1927 introduced models of general relativity that implied non-static solutions to the field equations, with Hubble introducing the law to measure the redshifting of galaxies in 1929. Once the idea of an expanding universe became a live option for scientists, it also became possible to go back to Slipher's experimental findings and to assess them as meeting the standards of performance adequacy of the scientific perspective of the time. For example, their consistency with non-static solutions of Einstein's field equations that Friedmann and other cosmologists were bringing to the fore in the 1920s. Using Supernova Ia techniques, current cosmologists can still assess the performance adequacy of Slipher's findings, despite his pioneering work being overlooked for a long time by his own peers, who had not yet precisified the standards of performance adequacy appropriate to their scientific perspective.

To conclude, success from within does not fall back onto success from above because ontologically true grounds *in and of themselves* (without also meeting standards of performance adequacy at the time) are necessary but not sufficient to license success. Scientific success is what a community of epistemic agents acknowledges and welcomes as such at any given time. The truth of the propositional contents of our scientific claims—the first conjunct in my premise (b)—by itself would grant only a view of success *from nowhere*, a view that no epistemic community (either *here now* or *back then*) would recognize as its own.

Success from within has both a realist and a perspectival component. Correspondence with perspective-independent states of affairs *and* meeting perspectival standards of performance adequacy (which can be assessed by other agents across perspectives) are *both* key to the success of our scientific claims (of today and of the past). Success from within is not the success of those who historically happened to be the winners. It is instead the success of those who were responsible for the

scientific findings *and* their ongoing performance adequacy as *still assessed by us today*.

(p.114) 6. *Envoi*

Fifteen years after “Real Realism” we are all heirs of Galileo’s strategy. We learned from Kitcher’s real realism how to tell truth from falsehood, how to discern bits that work from idle wheels, and most of all how to believe in the reliability of the deliverances of our instruments. Empiricists and constructivists of all stripes owe us an argument for maintaining a justifiable degree of skepticism about science and its success. More to the point, they owe us an argument for justifiably retreating into “metaphysical never-never-lands” on the face of so many homely arguments for being realists about the things with which we interact all the time.

For myself and for my generation real realism has enticed us to explore new avenues and encouraged us to appraise success and failure across the history of science in a careful way. Maybe success from above should leave room for success from within. We stand on Galileo’s shoulders by acknowledging our continuity with the past and our ability to assess past scientific claims by their own lights and from our current vantage point, a vantage point that is neither metaphysically nor epistemically privileged. That is how, in my view, a *perspectival* slant can help us bring real realism back to the Kantian home, to which it naturally belongs.

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Reply to Massimi

Philip Kitcher

Michela Massimi offers a subtle response to my real realism, in the spirit of the valuable—but these days all too rare—tradition of history and (p.115) philosophy of science. She begins with what she identifies as a problem for my views and proceeds to develop a form of perspectivism aimed at further articulating real realism. I agree that real realism might profitably be elaborated by incorporating perspectivist ideas and that this might enable a more adequate treatment of the science of the past.

It is helpful to start with a brief review of the difficulty that my notions of *working posits* and *idle wheels* were intended to solve. In his classic paper, Larry Laudan (1981) claimed that the confident realist defense of the approximate truth of "mature sciences," by appeal to the success of those sciences, was vulnerable to a straightforward historical challenge. According to Laudan, the scientific past is full of theories that are now regarded as false but that were taken to be true on the basis of their supposed successes. What right do we have to hold that our predicament is different? We look around and see success everywhere—but so did our predecessors, even though their theories were false.

In my 1993a book I offered a double reply to Laudan. First, I charged that his list of false theories successful in their own time was inflated. Second, I proposed that, in the instances of genuinely successful past practices, the theories in question were not entirely false—and the successes are to be explained by noting that they depend only on claims that remain, by our lights, true. Hence the thought that success depends on truth is restored. "Real Realism" (Kitcher 2001a) goes further by trying to locate the success-to-truth inference in the homely background of which both Massimi and I approve.

The divide-and-conquer strategy is to pick out the working posits of past science, the true claims and the genuine bits of nature to which our predecessors referred, seeing those as responsible for the past successes. These working posits are distinguished from the idle wheels, the constituents of past theories that played no positive role in successful practice. The example of Fresnel's wave theory of light is exemplary. Fresnel was correct to view light as having a wavelike aspect and to characterize wave propagation as he did. He erred in supposing that any wave motion must have a medium in which the waves are propagated and thus introducing an all-pervasive ether.

One of Massimi's worries is that this line of solution will not apply broadly enough. She cites the example of theories of free fall in the Aristotelian tradition. On her account real realism faces a dilemma: either one must import elements from later science into Aristotle's framework, or it's necessary to dismiss his theory as thoroughly false. The former option distorts the content of Aristotelian claims; the latter dismisses Aristotelianism too bluntly.

(p.116) I reply by trying to specify the exact claims at issue. In the Aristotelian tradition the terms we translate as "free fall" pick out the motions of bodies after their release as they tend toward the earth's surface. Some contexts will fix the reference of those terms by importing theoretical ideas from Aristotle; for example, free fall will be taken to be a motion toward a natural place. On other occasions, however, thinkers in the Aristotelian tradition refrain from any such theoretical imposition: free fall is just the downward motion of a body that has been released. Call these the strong and the weak senses, respectively. Consider now the statement "Bodies in free fall accelerate as they approach the earth's surface." When Aristotelians make claims we properly translate by this statement (and, of course, they don't use *accelerate*!), everything turns on the sense we assign to *free fall*. In the strong sense, if the logical form is taken to be a universal generalization, the claim is vacuously true, since nothing satisfies the antecedent. In the weak sense, however, the claim is a true empirical generalization, one that might be used to make some modest predictive successes. On this reading Aristotelians are claiming (correctly) that, when released, heavy bodies gain velocity in their motion toward the earth. It's thus possible for real realism, in its original form, to endorse parts of the Aristotelian view as correct and as predictively successful in consequence of their correctness.

The approach just sketched appears to make progress with a problem Massimi raises for me, but it remains historically crude, unable to make sense of the subtle conceptual shifts that lead from Hipparchus through Buridan to Galileo (early and mature). But before I outline some measures for refining the historical treatment, introducing a form of perspectivism akin to Massimi's, it's important to see how the problem of understanding the conceptual shifts is different from that of responding to Laudan's actual skeptical argument.

Historians, and historically sensitive philosophers, often writhe when they encounter judgments about the truth of particular claims made in the scientific past, especially when that past is quite distant. By what right do we adopt a "view from nowhere," judging the correspondence of (say) Aristotle's words with nature? The answer is that real realists

don’t adopt a view from *nowhere* but a view from *right here*, and they do so because it’s forced on them by the skeptical challenge. Laudan and the antirealists who follow him introduce that view. Here’s the challenge: “You realists believe that success indicates truth. But there are many past successful scientific theories that you regard, *by your own lights*, as false, so success can’t reliably indicate truth.” Evaluating bits and pieces of past science as correct from the perspective of contemporary science is an intrinsic part of any response to Laudan’s celebrated argument.

(p.117) Massimi’s perspectivism steps sideways from Laudan’s actual challenge—although, as I’ll explain shortly, there’s something important that motivates her. Before attending to that motivation, however, I want to point out an interesting feature of her perspectivism. Her condition (b) seems to make a stronger claim than is pertinent to Laudan-style skepticism: the first conjunct (the truth of the proposition) is enough. Why, then, add the second, which might hold all pieces of past science hostage to some thoroughly misguided and regressive scientific perspective, one so badly informed that it judges all the rest to fail in their own terms? Perhaps the condition might be more plausible as a disjunction rather than a conjunction—an interpretive idea inspired by her remark about lack of success. (“A scientific claim proves inadequate (and hence unsuccessful) when the content of the claim is false *and* it fails to meet its own standards of performance adequacy *when assessed from another perspective*.” The negation of (b), as it stands, should be a disjunction.)

But this is to miss the insight motivating Massimi. She (reasonably) wants (b) to approach truth-claims via using other perspectives to judge the performance adequacy of the focal perspective by its own lights (avoiding the “view from nowhere” and denying any privileged role to the “view from here”). As I’ve said, to proceed along those lines fails to mesh with Laudan’s skeptical argument as actually posed. On Massimi’s view, as I understand it, Laudan’s deep interest in destabilizing our contemporary views should lead him to be suspicious of the notion of success he actually adopts in his critique of convergent realism: he should not be content with evaluation “from above” but should ask the realist to show that success depends on truth, when the truth of elements of past science are assessed from within. The convergent realist (or the real realist) ought to show that the evaluation is stable as one proceeds from the original practice through the sequence of practices that succeed it. In this way both the critic and the realist avoid taking any standpoint as privileged. Massimi claims that Laudan’s challenge should have asked for something stronger, namely

the stability of the judgments about success and truth under evaluation from within. Combining my crude original approach to Aristotelian language with the more subtle schemes of translation available from perspectives intermediate between Aristotle and us (e.g., those of Hipparchus, Buridan, and others), I think the realist can meet this stronger challenge.

In my view the deep motivation for introducing the idea of evaluation from within stems from Kuhn's seminal reflections on the languages of past science. When you examine the subtle ways in which later Aristotelians discussed free fall (reviewed in some of Massimi's illuminating historical studies), the translations I offered in evading her original dilemma seem (p.118) remarkably blunt. Following Kuhn (especially 2000), we might declare the central claims of Aristotelians to be literally untranslatable. I'd prefer to put the Kuhnian point differently: we can arrive at approximative translations, highly context-dependent and requiring preliminary glosses to show the ways Aristotelian and modern terms cut across one another. Neither the Kuhnian version nor my preferred alternative vitiates the basic point that Aristotle's successors achieved an insight we can best capture with the formulation "Bodies in free fall accelerate as they approach the earth's surface"—and it's precisely by attributing that insight to them that Laudan-style skepticism is answered (in the particular instance).

Real realism needs extension, in my view, because it should appreciate the limitations of the translations advanced in combating skepticism. Massimi is right to suppose that, through a sequence of theoretical developments of a large perspective (say, Aristotelianism), the more immediate descendants are more able to reconstruct and explain the ideas of their predecessors than those who come later: *once the working posits and idle wheels have been identified* (by us!), Buridan is a better interpreter of the terms used by Hipparchus in characterizing them than is Galileo, and Galileo, in turn, does better than we can. If we can attribute a core insight about acceleration toward the earth (as I think we can), we must also recognize that the Aristotelian terms in which that insight is expressed are alien to us and that they are less strange to those who are closer to the Aristotelian worldview.

Recognition invites the idea of a sequence of perspectives, distinguished by ways of conceptualizing the phenomena (in this instance, phenomena of motion), in which close successors are better able to capture the claims of their predecessors. To rebut Laudan's skepticism it's enough to show that where there's success there's an underlying use of correct ideas, often expressed in what much later scientists see as highly peculiar ways. A much deeper understanding of

the phenomena of success, and how it's won, requires the historian-philosopher-of-science to reconstruct the perspectives of the past, tracing their continuities with the present.

I read Massimi as aiming to avoid any privileging of the contemporary standpoint, the "view from here." For me, the principal motivation for perspectivism stems from the shortcomings of the translations used in reconstructing the successes of past science. In the rest of this response I'll trace a route to my preferred version of perspectivism.

Massimi draws from Kant (as I once did). My sources these days are the classical pragmatists. On my interpretation of Peirce, James, and Dewey, none of them rejects my favorite (post-Tarskian) version of correspondence truth for scientific statements (see Kitcher 2012c, ch. 5). Yet James (p.119) and Dewey (especially) are struck by the plurality of frameworks in which scientific truths can be embedded. Both of them view the world—in the rich sense in which the world contains determinate objects divided into kinds—as partially constructed by the community of inquirers, with some features of that construction reflecting aspects of human psychology and others responding to our evolving interests. So at different stages in the development of a science, alternative languages will be articulated, specifying different spatiotemporal boundaries for objects, different groupings of things into kinds, different termini for processes, and different standards for normality. None of these languages is privileged in the sense of conforming to the intrinsic structure of an independent reality. Conceived as what is independent of the subject, reality doesn't come with that much structure. With respect to different human purposes, however, some languages may function better than others.

A thorough evaluation of some past perspective would start with reconstructing its language, exposing the ways its categories cut across those of the present. It would proceed to delineate the goals at which inquiry aimed, what questions investigators selected as especially significant, and what standards were adduced for answering them. On this basis it would explore the extent to which those investigators succeeded in attaining their goals, why they were successful when they were, and why they failed when they did.

So far the historian-philosopher-of-science is using the contemporary perspective to study and to appreciate a past perspective in its own terms. Mindful, however, of the large degree to which the languages cut across one another, with consequent inadequacy of translation, a historically sensitive philosopher may explore the intermediate perspectives, using the more immediate descendants as superior guides

to the structure of the ancestor. In the end the full sequence of perspectives may serve as the most adequate standard against which ancestral success is judged. If I am right, that conforms in important respects to the perspectivism Massimi prefers. It is, however, couched in a different philosophical idiom and directed at a different philosophical problem.

In my view real realism fights on many fronts. As my 2001a paper tries to show, real realism opposes several varieties of empiricism and constructivism. Laudan's skepticism is akin to one strain of empiricist argument, and it is among the objections I have been concerned to rebut. My current view, however, is that Kuhn's ideas about conceptual incommensurability, particularly as they were elaborated in his later writings (Kuhn 2000), offer an equally important challenge, demanding of real realists that they offer a more nuanced view of scientific success, past and present. (p.120) The neopragmatist perspectivism I have sketched attempts to take up that challenge.

In the end, I think, a perspectivalist real realism is doubly motivated—and we don't have to choose which rationale is more important. Massimi views Laudan's use of evaluation from above as betraying one of his central insights; thus she introduces a more probing account of success and elaborates real realism with respect to it. I have been more troubled by a Kuhnian challenge. But we come out in much the same place. This is not so much because of the resurgence of my lapsed Kantianism as through celebration of a characterization Kuhn came to relish: he was redoing Kant with movable categories. So too, I believe, were the classical pragmatists. Real realism should continue the enterprise.

Notes:

(1.) Against the epistemological constructivists of Kantian descent invoking a "distinction between objects-as-experienced and objects-in-themselves" (*RR* 189), real realism responds that "the objects we claim to represent accurately are not mysterious noumena but, in many cases, the things with which we interact all the time" (189).

(2.) See, for example, Sosa's perspectival coherentism (part of his virtue perspectivism in Sosa 1991), where the justification for beliefs is a matter of perspectival coherence. Along similar lines, on perspectival justification for beliefs, please see Haack (1993) and Rosenberg (2002, 149): "The reason that we correctly judge that *S* does not know that *p* is that, given our richer informational state, we recognize that what we are (stipulatively) entitled to take to be *S*'s epistemic circumstances demand a higher level of scrutiny than we are supposing *S* himself to have exercised. *S* therefore, has not satisfied what, from our perspective, are the standards of performance-adequacy appropriate to his epistemic circumstances, and hence, from our epistemic perspective, we judge that, despite his not having acted irresponsibly given the information available to him (judged from his own legitimate perspective on his epistemic circumstances), he has not justifiably come to believe that *p*." In what follows I latch onto and expand upon Rosenberg's appeal to standards of performance adequacy, but in a different context and with a different purpose in mind. My goal is not to elaborate a perspectivalist theory of belief justification but instead to elaborate a perspectivalist notion of *success from within* that can serve the purpose of success-to-truth-inferences in the realism debate.

(3.) Here I want to latch onto the helpful distinction between context of use and context of assessment in discussions on relativized truth and faultless disagreement. See MacFarlane (2005, 2009) and Marques (2014), among many others. By contrast with MacFarlane, I will not be using this distinction to defend any notion of relativized truth. Instead I make use of MacFarlane's distinction between context of use and context of assessment to provide a notion of success in science that does not beg the question for scientific realism (i.e., that does not judge past theories on the basis of our current successful theories).

(4.) On closer reflection, this is what is to be expected from Kuhnian anomalies and periods of crisis. Anomalies reveal cracks in well-established and well-trodden paradigms by revealing the inability of the paradigm to handle *in its own terms* an increasing number of persistent problems.

(5.) Following up on Thomson, in 1850 Rudolf Clausius laid the foundations of thermodynamics (let us call it SP_2) by reconciling Carnot's cycle with Joule's ideas. For Clausius envisaged that it was possible to retain Carnot's idea that heat passes from a hot reservoir to a cold one whenever mechanical work is done in a cyclic process, while also abandoning Carnot's additional claim about conservation of caloric. The second law of thermodynamics was born: in any cyclic transformation of thermal energy into mechanical energy, a portion of heat gets dissipated irreversibly (pace caloric theory). Subsequent perspectives, such as Maxwell-Boltzmann statistical mechanics (let us call it SP_3) were still able to evaluate the truth of Carnot's cycle and its ability to meet standards of performance adequacy in its own time (i.e., measuring engines' efficiency in producing mechanical work). Yet in the light of the richer informational content available to Maxwell and Boltzmann (after Clausius's introduction of entropy), Carnot's overall claim was deemed as requiring a higher level of scrutiny (about conservation of caloric and the nature of heat) than Carnot himself could have possibly exercised in the early nineteenth century.

(6.) Here a relativist may come to the fore and make this kind of rejoinder. (One is reminded of the familiar story about the Azande and their witchcraft and how standards of adequacy vary from one epistemic community to another. See Kusch 2002 for a helpful discussion.) A discussion of relativism would lead me into territory farther afield from the topic of my essay here, and as such I will not pursue it.

(7.) Boyd (2010, 217–18) describes the hypothetical scenario of the lone researcher who gets it right, but she does not make any contribution to the reliability of our scientific practice unless her success is also recognized as such by a community.

(8.) Here I draw on John Peacock's account of this episode in Massimi and Peacock (2014).